

Peer-review report of

Borghi, O., Niraki, M., Seremeta, E., Smets, K., & Tsakiris, M. (2025). Facing a dark future: Young people's future anxiety and political attitudes in the UK and Greece. *advances.in/psychology*, 2, e555124. <https://doi.org/10.56296/aip00042>

Round 1

Dear Authors,

Thank you for submitting your important work, "Facing a Dark Future: Young People's Future Anxiety and Political Attitudes in the UK and Greece," to our special issue, *The Psychology of Pushback*. We were fortunate to receive detailed evaluations from two leading experts in the field, and we appreciate their thorough reviews. The reviewers bring a range of expertise to their assessments, and their comments are detailed and constructive. Before consulting their reports, both editors also read the manuscript in full.

We see several clear strengths in your study. It addresses an issue of both theoretical and practical importance with a novel perspective. The manuscript includes rigorous analyses, compelling visualizations, preregistration, and the use of large, stratified samples. Key findings are replicated across two national contexts. The manuscript is also very well written. Your work aligns well with the goals of the special issue, and it has the potential to attract a broad audience. We invite you to revise the work for potential publication.

That said, both reviewers identify matters that need to be addressed for further consideration for publication. Because their comments are clearly presented, we will not repeat each point here, but we ask you to address them fully in a revised version. Here we highlight several of the substantive points made by the reviewers and offer some additional ones based upon our independent reading and review of your work.

One reviewer expresses concern about the deviation from the preregistration that led to the inclusion of a second sample. This addition was made at our request when we learned that data from this country were available. We therefore ask that you retain these results but, as the reviewer suggests, provide a stronger justification for including the second context and discuss any differences between the samples—mindful that metric equivalence was not achieved.

Another fundamental issue discussed by Reviewer 2 is one that we noted in our preliminary review of your work – the limitation of cross-sectional survey designs. You clearly acknowledge this limitation and discuss important interpretational issues. As Reviewer 1 remarked, "the discussion is well-structured and thoughtful. I especially appreciated the consideration of opposing causal directions." In this case, we do not see the cross-sectional design as necessarily a fatal flaw in the current manuscript, but it makes developing a stronger and fuller theoretical rationale that much more important.

Reviewer 1 identifies several ways that the current theoretical case can be strengthened. The Reviewer explains that “the theoretical justification for the inclusion of the political variables of political ideology, authoritarian attitudes, and democratic attitudes’, as well as the cognitive variable of dogmatic, evidence-resistant thinking remains underdeveloped,” and that “a theoretical model related to cognitive reappraisal versus expressive suppression should be mentioned.” We concur and ask that you develop the theoretical foundation and justify your chain of logic for the hypotheses more fully in the revised version of the work. Also, as Reviewer 1 notes with respect to the *Actively Open-Minded Thinking* (AOT) scale, it is important to be more precise in how you apply key concepts (such as “dogmatism”).

Reviewer 2 raises an important conceptual issue that we did not originally consider but which we believe is important to address. The Reviewer explains that the distinction between anxiety and future anxiety is critical to the interpretation and meaning of the findings. Accordingly, we recommend, as Reviewer 2 proposes, that (if the data are available) you might “simultaneously control for participants’ general mental health, well-being, or anxiety (or any proxy for such)” to make the case for the “future” element of the anxiety. If those data are not available, please elaborate on this distinction between anxiety and future anxiety more in the Discussion, giving readers concrete guidance about how these can be disentangled empirically in future research.

In addition to these points, please address the following:

1. In the abstract, change “future anxiety was also associated with political participation.” to “future anxiety was also associated with higher political participation.”
2. Please describe in the main text how subjectively perceived living standard (and how it was recoded) and gender were measured. Same goes with education.
3. Explain why you controlled for the various demographic variables (also see the comment by one of the reviewers). Report whether findings were robust to not controlling for them (this can be summarized in the main text and be presented in the SOM in details).
4. Please mean score the future anxiety scale to increase interpretability.
5. Please keep all of the current results in the main manuscript (against the suggestion by one reviewer). However, please make sure to report statistical information also for non-significant effects that are described in the main text or are relevant (e.g., “Our model indicated no credible associations of future anxiety with political ideology or authoritarian attitudes, with 95% CIs including zero and posterior distributions substantially overlapping the ROPE (see Supplementary Table 5a), and only a very small positive association with dogmatic attitudes ($\beta = .07$, 95% CI [.002, .13]).” This applies to several places in the results section.
6. There are a few minor stylistic issues:

- In line with APA7, please don't italicize example items (however, endpoints should be italicized as you currently do).
- Please follow APA7 capitalization norms for headings.
- Please italicize statistical Latin letters (does not apply to Greek letters).
- Some of the reliability estimates may be better described as “acceptable” instead of “satisfactory.” Please check with established standards and consider changing the wordings for some of the scales.
- Please provide the figures as SVG files entered into the word documents to ensure high resolution.
- Do not capitalize the word “see” when referring to figures.

Thank you again for submitting this important work to our special issue. Despite some limitations to the work (e.g., the cross-sectional design), we found the research novel and the findings provocative. It has the potential to stimulate future work in this area. Thus, we hope that you accept our invitation to revise the manuscript for further consideration for publication – informed and guided by the two excellent reviews. The reviewers contributed considerable time and effort to identify ways to strengthen your work. Given that we are not requesting additional data collection but mainly revision of the narrative and possibly some additional analyses, we look forward to receiving a revised version within a month's time (i.e., by August 31). Please outline in a revision letter how you addressed each point and provide excerpts that outline these changes.

Best,

Jonas R. Kunst & John F. Dovidio

Reviewer 1:

I was invited to review the manuscript titled “Facing a dark future: Young people's future anxiety and political attitudes in the UK and Greece” for potential publication in advances.in/psychology.

This work is a cross-sectional online survey study investigating the relations of adolescents' future anxiety with various political views and one cognitive characteristic, including the potential moderating role of two emotion regulation strategies. The study draws on two adolescent samples: one from the UK (N = 988), and one from Greece (N = 997), in which some of the scales were assessed as well. Among the key findings from the UK data is a positive association between future anxiety and “democratic attitudes” (i.e., endorsement of democratic principles), which was moderated by cognitive reappraisal.

There is much to like about the present manuscript. The topic is timely and societally relevant, addressing how young people's psychological experiences are linked to their political views. The manuscript is also written in good English. The

preregistration of the UK part of the present study and the provision of materials via the OSF increases transparency. The inclusion of a second sample from Greece to replicate some of the analyses strengthens the study's potential generalizability.

However, the manuscript also exhibits several limitations and issues that need improvement before it can be considered suitable for publication. I outline these points in detail below, and I hope the authors will find it helpful.

Abstract:

The mention of emotion regulation strategies as moderators comes a bit surprising in the methods section of the abstract. Including a brief theoretical justification earlier, i.e., a sentence indicating why emotion regulation strategies may shape the relations between future anxiety and political views, would help readers better understand the approach.

Currently, the term "democratic attitudes" is somewhat vague. It would be helpful to specify whether the construct refers to support for democratic principles, satisfaction with democracy, or another more specific concept.

The abstract should also clearly state that only a subset of the measures was assessed in the Greek sample. This is essential for transparency, especially given that the cross-national replication is highlighted as an advantage of the study.

Introduction and Theoretical Background:

In these paragraphs, I see the greatest weaknesses of the present work. More specifically, in its current form, the theoretical justification for the inclusion of the political variables of political ideology, authoritarian attitudes, and "democratic attitudes", as well as the cognitive variable of dogmatic, evidence-resistant thinking remains underdeveloped (see page 3). The manuscript would benefit from a more robust theoretical and empirical rationale explaining why these particular constructs are expected to be related to future anxiety among adolescents. If no single theory explicitly addresses these links, drawing on related or broader theoretical frameworks could be beneficial.

Examples are the works by Ronald Inglehart (1971: 10.2307/1953494; 2015). Alternatively, Duckitt and Sibley (2010: 10.1111/j.1467-6494.2010.00672.x) propose that the worldview of a dangerous world underlies right-wing authoritarianism. Moreover, various works link perceived deprivation to extreme political ideology (e.g., works by Kunst and Obaidi and colleagues: 10.1016/j.copsyc.2020.03.010; 10.1177/0956797619834879; 10.1080/10463283.2025.2478735). Additionally, the work by John Jost (e.g., 2003: [10.1037/0033-2909.129.3.339](https://doi.org/10.1037/0033-2909.129.3.339)) links conservative ideology to needs, including security-related needs.

While I am aware of the fact that none of these theories directly addresses the relations of relevance to the present work, some of them may nevertheless be helpful to generalize and carve out a theoretical rationale for the relations examined here. If other theories are available that are more closely related to the present work, these should be cited, accordingly.

Similarly, a theoretical model related to cognitive reappraisal versus expressive suppression should be mentioned.

After improving the theoretical and empirical rationale of the present work, also the research questions and/or hypotheses could be highlighted more in the text.

Page 2: The statement “Amidst all these, a key question that remains unanswered is how young people’s anxiety about what the future holds relates to their politics.” is extremely broad. I recommend specifying which political dimensions are of interest; e.g., ideological self-placement (symbolic ideology), support for democratic vs. authoritarian values, etc. A similar clarification is needed throughout the work, for instance, on page 4, line 76.

Page 2: The sentence “Expectations of socioeconomic downward mobility are one of many factors that can contribute to future anxiety.” requires a citation to support this statement.

Finally, I suggest removing methodological explanations from the background sections (see the last paragraph before the methods section).

Methods:

First up, I recommend clearly stating the study design at some point (i.e., cross-sectional online survey).

Further, for interpretability of the sum scores and descriptive statistics, please indicate the number of items used in the future anxiety scale in the *Methods* section, not only in the Results.

Page 5: It is stated that “Sample sizes were pre-determined at the start of the larger project based on economic considerations”. This is understandable but somewhat vague. I suggest clarifying what specific constraints, cost calculations, or power analyses were used to set sample sizes within the manuscript. Furthermore, it would strengthen the present work to report a post-hoc sensitivity analysis indicating the minimum effect sizes that could be reliably detected with the given samples. This would help readers interpret the findings in light of statistical power.

For full transparency and reproducibility, I suggest listing *all* scales and items used in both surveys. If space constraints prevent this in the main manuscript, a complete overview in the OSF repository (with a link provided in the manuscript) would be helpful.

The manuscript would benefit from a description of how the Greek versions of the scales were derived; e.g., were they derived from previous publications, or translated using a forward–backward translation protocol?

I was extremely surprised to learn that an Actively Open-Minded Thinking (AOT) scale was used, yet the theoretical and methodological framing refers to “dogmatism” or “dogmatic attitudes”. This – for me – constitutes the second major weakness of the present work next to the lack of a through theoretical rationale mentioned before. While AOT and dogmatism are inversely related, they are not conceptually identical; as also acknowledged by the authors through presenting a moderate negative correlation between the two constructs. Given this, I strongly recommend (i) clearly defining the constructs of interest (AOT vs. dogmatism), (ii) ensuring consistent terminology throughout the manuscript, and (iii) integrating relevant theoretical background on AOT in the introduction if it is the primary construct of relevance. If, however, the theoretical interest lies in dogmatism, it may be worth discussing why a dogmatism-specific scale (e.g., Altemeyer’s Dogmatism Scale or others; like the one published by Shearman & Levine (2006: 10.1080/01463370600877950) was not used. In addition, this must be mentioned in the discussion section as a major limitation.

Page 8: I am not an expert in Bayesian statistics. However, the use of “*weakly informative priors of $N(0, 1)$* ” is mentioned. Given this specification, it is unclear to me what theoretical or empirical rationale led to the use of a Bayesian approach in the first place. It would be great if it were clearly specified what advantages or inferential goals the Bayesian framework was intended to address in this context.

Finally, a brief rationale for the inclusion of each control variable would strengthen the methodology.

Results:

Overall, the results are presented in a transparent and easy-to-understand way.

Page 11: “[...] was moderate-to-high ($M = 23.47$, $SD = 6.79$).” ; “[...] future anxiety (measured 265 with 4 items; possible range: 4-28) was moderate ($M = 17.59$, $SD = 5.22$).”

Without the availability of large-scale normative data or validated cut-offs, such descriptors can be misleading. I recommend removing interpretive phrases such as “moderate” or “moderate-to-high” and instead reporting the scores descriptively.

There are a lot of results presented that were not in the main scope of the present work, and that were not preregistered. While these are potentially valuable and interesting, I recommend relocating them to the Supplementary Material. Doing so will help maintain a clear focus on the preregistered research questions and hypotheses in the manuscript and more transparently distinguish confirmatory from exploratory findings.

The methods used in the paragraph “*Which young people are anxious about the future?*” are unclear. Were the analyses conducted using the same Bayesian modeling approach as the primary analyses, or were “normal” regressions used?

Discussion:

As mentioned in relation to the Results section, I suggest avoiding characterizing future anxiety levels as “moderate-to-high” or “moderate” without normative data.

I also recommend briefly addressing key limitations, such as the cross-sectional design (limiting causal inference) and reliance on self-report measures. A short comment on the generalizability of findings across countries would further increase transparency.

That said, the discussion is well-structured and thoughtful. I especially appreciated the consideration of opposing causal directions.

Reviewer 2:

This is a well-crafted paper that demonstrates several considerable strengths. It addresses interesting and societally relevant research questions, is pre-registered, well-written, and reports and discusses results based on Bayesian inference with clarity and a high level of analytical competence. However, despite these undeniable advantages, the paper has several significant shortcomings that, in my view, prevent it from being published. Here are my main concerns:

My most important concern relates to the main finding: future anxiety is, contrary to expectations, associated with greater support for democracy and higher political participation. I agree with the authors that the causal relationship likely runs in the opposite direction, suggesting that those who are more democratically engaged may fear the future of democracy more. However, this is such a crucial point that it needs to be tested empirically before drawing any conclusions. The authors describe their dataset as a longitudinal panel, so this issue might be addressed at a later stage by considering temporal ordering of variables (using outcomes from time t and lagged predictors of future anxiety from time $t-1$). Unfortunately, as the authors argue, it is also something that cannot be tested with the dataset used in the study. I am concerned that, without clarification of this point, the potential contribution of the paper is rather limited.

Another major point relates to the main predictor, future anxiety. While it is a validated scale, unless the authors can simultaneously control for participants' general mental health, well-being, or anxiety (or any proxy for such), I am not convinced that the ‘future’ aspect of anxiety is chiefly responsible for the observed associations. After all, the authors acknowledge this limitation in the discussion.

My final major issue pertains to the insufficiently justified deviation from the pre-registration protocol regarding the inclusion of the Greek sample in the analysis. It is unclear why this additional case should be analyzed alongside the pre-planned analysis of the UK data, and there is no discussion of what the differences in results may imply for the research question. The reader does not learn anywhere why exactly these two cases were selected and what their comparison should reveal about the substantive problem (was it for convenience, or do they represent contrasting cases, most similar cases, etc.)? The lack of scalar invariance for future anxiety between the two countries and the missing items in the Greek sample (e.g., emotion regulation items, authoritarian attitudes, dogmatic attitudes) further complicate this choice. I do not want to make assumptions, but from a reader's perspective, it almost appears as if the authors decided to augment the analysis only after their expectations regarding the association between future anxiety and political attitudes did not materialize in the UK data (notably, there is an association between future anxiety and right-wing ideology in the Greek sample). I believe the authors would be better served by focusing solely on the UK data, acknowledging that the data do not support their expectations, eliminating the Greek sample entirely, and rewriting the paper with only the UK participants. Including these two samples with diverging results is problematic, especially given the absence of an institutional or cultural rationale for the differing results in these two contexts. Removing the Greek data would enhance the paper's credibility and significantly improve its readability, as currently, the reader must navigate between Greek and British results, which is less than ideal. On the other hand, I do not see any issue with extending the analysis to include testing moderation by gender (which needs to be better theorized in the introduction, as it currently relies on only one unpublished paper) and political participation as an outcome. I would encourage the authors to also test for moderation by emotion regulation strategies, as is the case with all other outcomes.

Lastly, I was somewhat surprised by how the authors constructed the political ideology scale by combining left-right and liberal-conservative scales into one. I believe it would be more insightful to analyze them separately.

As I mentioned at the beginning, this is a well-written paper, but due to the unresolved issue regarding reverse causality, I am unfortunately leaning towards a rejection.

Round 2

Dear Authors,

Thank you for submitting your revised manuscript "Facing a dark future: Young people's future anxiety and political attitudes in the UK and Greece" to The Psychology of Pushback. We were fortunate to receive evaluations from the same two experts who evaluated the first version of your work.

We all agree wholeheartedly that your work has improved significantly. Thank you for your diligence in addressing our concerns and for taking the time to clearly communicate the changes with us. We believe that the paper is now very close to reaching the acceptance threshold and that only a few minor changes will be required.

One of the reviewers outlines some minor changes that can further lift your paper. In addition, we suggest splitting the methods description for "Support for Authoritarian and Democratic Principles" into two sections, given their drastically different content.

While your graphs are great, please center the y-axis label for Figure 2 ("Association with Future Anxiety") and Figure 3 ("Predicted estimate") and ensure consistency in terms of capitalization across all graph elements.

In terms of deviations from the pre-registration, please clarify that these were due to requests from the editors (inclusion of Greece) and reviewers (e.g., AOT naming). This will make it even clearer that these were not strategic changes from your side but externally induced.

We believe that these changes will require minimal additional work. To expedite the publication of this important work, we request that you submit a revised version within two weeks, i.e., by October 15. Please reach out to us if you require additional time.

Best,

Jonas R. Kunst & John F. Dovidio

Reviewer 1:

I have carefully re-read the revised version of the manuscript "Facing a Dark Future: Young People's Future Anxiety and Political Attitudes in the UK and Greece". The authors clearly invested a substantial amount of time and effort into addressing the comments from the editor and reviewers, and this has led to a notable improvement of the manuscript.

I do, however, have a few additional suggestions that I believe could further strengthen the paper:

The theoretical background has become much stronger and more coherent. It would, nevertheless, be helpful to emphasize more explicitly that the present work examines relationships that have not yet been directly addressed in the theories and empirical evidence cited. Theories mentioned are relevant, but they do not specifically address the variables at the core of this work. Making this point clearer would highlight the originality and added value of the study.

Next, in the abstract, the study is framed in terms of "political attitudes" as dependent variables. However, the manuscript also investigates actively open-minded thinking (AOT). Since AOT is not itself a political attitude, I recommend clarifying this distinction.

This comment does not relate to the introduction of the concept of AOT, but rather the abstract and whenever the authors refer to “political attitudes” to refer to their dependent variables (e.g., when introducing the research questions/aims and in the methods section).

Since the preregistration is about the dogmatism concept, I recommend including a short note/footnote in the theory sections that the paper focuses on AOT contrary to the concept presented in the preregistration (but not contrary to the preregistered measurement instrument).

Finally, I recommend including a summary or conclusion in the end of the manuscript – just a very short paragraph to not end on the constraints to generalizability of the present work, i.e., on a negative note.

Reviewer 2:

I would like to thank the authors for their extended revisions in response to my comments. I appreciate the conceptual and theoretical choices made, as well as more transparent communication regarding their data-selection choices. While my main concerns remain the same (as they could not really be addressed with the data at hand), I want to acknowledge that changes that the authors implemented did substantially improve the paper.